Draft Individual Review Form

Proposal number: 2001-C212-3 **Short Proposal Title:** Large-scale…Flow and Sediment Transport

1a) Are the objectives and hypotheses clearly stated?
1b1) Does the conceptual model clearly explain the underlying basis for the proposed work?
1b2) Is the approach well designed and appropriate for meeting the objectives of the project?
1c1) Has the applicant justified the selection of research, pilot or demonstration project, or a full-scale implementation project?
1c2) Is the project likely to generate information that can be used to inform future decision making?

Singer proposes a comprehensive study to develop a model of sediment movement in the Sacramento River channel using a combination of historical information on hydrological and geomorphic parameters and his own field-collected data. The context of this study if very important to ERP goals, particularly as sediment transport and deposition is critical to successful restoration of geomorphic features and riparian vegetation (and associated wildlife), and an inadequate evaluation of the quantity and timing of sediment routing could jeopardize some of these restoration projects. The research potentially addresses both deposition and erosion, which may occur where sediment sources or transport mechanisms are inadequate to maintain a fluvial (so-called) 'quasi-equilibrial' sediment budget. The 'related research' basis for his work was well-developed and clearly explained the value of what is essentially state-of-the-art empirical, predictive sediment modelling research for large-scale systems. While this is clearly good research that is proposed, there is also an element of risk involved in what is a fairly theoretical approach with a huge basin being modelled. If CalFed chooses to support basic research that may ultimately have potential for wide application, then this is the type of synthetic work that would fit this bill.

My concerns primarily involve the scale of the project that seems to be proposed in a somewhat independent fashion. Many other workers are conducting related work in the Sacramento Basin, and while I'm sure that the potential for co-ordination is there, it is unfortunate that the research is not more closely tied to actual in-stream projects that would provide useful settings for model validation. By operating on the scale of almost the entire Sacramento drainage there is limited (but possibly critical) opportunity to really evaluate empirical sediment dynamics that are operating on local spatial scales. Perhaps the proposer could consider working more closely with restoration managers along the River who could contribute in this manner. Then again, there is an inherent conflict between constructing a model which operates on a very large spatial scale and then applying it on a small scale (e.g. a several acre riparian revegetation project) where local factors seem likely override regional dynamics in an unpredictable manner (this is partially taken into account through the stochastic parameterization that is apparently to be employed). In addition, while Singer has conducted preliminary research on Sacto. R. hydrology, this remains a relatively new system to him, and it might be appropriate to consider pilot evaluations be first conducted on a smaller areal extent. Or, given the extreme complexity of variables impinging on hydrology/geomorphology in this river and the huge logistical considerations that must be involved in this research, perhaps another system which represents low gradient floodplain dynamics but on a smaller spatial scale could be attempted first in order to develop the model that could eventually be applied to the whole basin; the Tuolomne R. or the Merced R., where substantial baseline geomorph. data also exist and some restoration projects are in progress or proposed could provide such model systems, as could several other major tributaries that enter the Sacto. or San Joaquin mainstems. A more localized project like that also seems to offer better potential for collaboration with hydrologists and restoration ecologists to estimate and test model parameters. I am not suggesting that the project, as proposed, is not feasible nor advisable, but it seems that model development and testing could be done in a more cost-effective manner on a smaller scale watershed, particularly if development is not successful. If such a project could be funded as a pilot or demonstration project, then research could proceed and, if successful, fuller-scale implementation could be supported at a later date. I would be particularly interested in a project in which system dynamics could be tested in the sediment routing model in a BACI-type (Before/After, Control/Impact) experimental design, particularly as it forms the basis for the proposer's doctoral research program.

2a) Are the monitoring and information assessment plans adequate to assess the outcome of the project?
2b) Are data collection, data management, data analysis, and reporting plans well-described, scientifically sound and adequate to meet the proposed objectives?

This is in some ways the weak element of the proposed research, particularly integrating results of model development with resource management. I discount to some extent the overall importance of immediate application of the project results, since this project is more research-focused than typical CalFed proposals, but it is still not clear how model validation will be accomplished. following the parameterization. The 'Expected Output' is reasonable, esp. given the long-term (decadal) relevance of the processes involved, but the linkage between most-likely outcomes and actual field conditions was not particularly strong. This is where a closer collaboration with other workers would be helpful, so that model application and testing could go hand-in-hand with empirical results.

3) Is the proposed work likely to be technically feasible?

This modelling effort is obviously a long-term proposition, and it may be unreasonable to assume (both by the proposer and by the funding agency) that a 3-year project will yield the comprehensive and robust 'predictive model' that is anticipated. Annual variation in degree and timing of discharges, and all the other natural and management 'contingencies' that take place in this watershed will certainly mean that a longer period of time will be needed to build comprehensiveness. Again, I don't expect immediate results for basic research (with applied application), but maybe this reality should be more fully discussed.

The proposed research approach itself is strong and seems to be technically sound, although to carry it out for this large basin is a huge undertaking for a graduate research project.

4) Is the proposed project team qualified to efficiently and effectively implement the proposed project?

Singer has conducted some interesting work related to large river hydrology, although the lack of a large research and publication record is a bit of a concern. On the other hand his advisor, Thomas Dunne, is a leading authority on sediment transport and large river hydrology, and will presumably be intimately involved in this research even if not based in the region. Singer has 'communicated' with USGS researchers involved in Sacto. R. sediment dynamics, but this does not constitute a 'project team'. I reiterate my concerns that this research could be strengthened if it were more fully integrated with related academic and agency research and management, whether in the Sacramento mainstem or in a tributary floodplain that might provide a more tangible study system.

Miscellaneous comments

Singer is probably correct in saying that 'there are no comprehensive...process studies of hydrology and sediment transport...in the Sacramento basin', but if that is the case then it makes the undertaking appear more risky than it probably is. This strong statement should be countered by more directly explaining that related studies elsewhere (and here) provide a research base which increases the likelihood that this modelling effort will be successful.

I'm wondering whether it is too early in the regional restoration program to put together this model. If there are to be lots of projects that will theoretically interact with each other (via water and sediment transport), then maybe this research should proceed more slowly and more fully integrate these projects. In other words, maybe the research could complement, rather than drive, restoration projects, since it is somewhat optimistic to believe that the results of this research will fundamentally determine where and how restoration work will occur.

Reflecting my own disciplinary bias, it seems incomplete to focus solely on sediment routing models, when one of the key goals of the ERP is to promote natural processes in riparian ecosystem. I would have appreciated a more ecological discussion of model application that would take into account how desirable riparian plants establish and develop in relation to fluvial dynamics. I concede that physical processes are undoubtedly more fundamental to dynamics than are biological processes, but this is not a holistic view in light of the CalFed emphasis on the ecological importance of riparian vegetation.

Returning to the subject of watershed-wide stasis in the Sacto. system, what are the implications of long-term processes that are relatively insensitive to annual discharge, such as the gold-mining 'pulse' moving through the system? How does the model account for large (important) but unpredictable events or system failures, such a dam or levee breaches?

Is it practical to measure ‘maximim scour depth’ in a channel of this dimension? Maybe, but I am skeptical. How does scouring depth interact with sedimentation rate estimates using the core radionucleotides? By the way, I really like the discussion of floodplain modelling that incorporates real and theoretical ‘ponding’, and maybe vegetation could be incorporated into this component as this sort of a roughness factor.

Overall Evaluation Summary Rating

- ☐ Excellent
- X ☐ **Very Good**
- ☐ Good
- ☐ Fair
- ☐ Poor

Provide a brief explanation of your summary rating

Very Good This is good quality hydrological science that is proposed, and could provide long-range information on site-specific sediment deposition and erosion potential. Reservations include possible risk of failure or incompleteness in the modeling effort within the timeframe proposed, as is the case in any novel research program, and inadequate integration with related research and management programs in the region.